

NARROWING THE GAP BETWEEN FIELD STUDIES AND LABORATORY EXPERIMENTS IN SOCIAL PSYCHOLOGY: A STATEMENT BY THE SUMMER SEMINAR*

THE following statement is an attempt by the members of the 1954 summer seminar to pull together the main issues in field and laboratory research that were discussed at the seminar. The statement is, of course, not exhaustive, since it is based on observation and reflection rather than on a systematic search of the literature and painstaking analysis. The subject is large and has many facets; this is only an initial statement, not a final word.

NATURE OF THE GAP BETWEEN FIELD AND LABORATORY STUDIES

Control. The chief difference between laboratory experiments and field studies lies in the degree of control exerted over the situation and events being studied. This factor of control can be used as the defining distinction between the two kinds of investigation, ignoring the usual discrimination based on the place where the research is carried out.

* The members of the interuniversity summer research seminar on field and laboratory studies, held at Brandeis University from June 28 to August 7, 1954, were Richard Christie and Morton Deutsch, both Associate Professors of Psychology, New York University; Nicholas J. Demerath, Research Professor, and John W. Thibaut, Associate Professor of Psychology, University of North Carolina; Leon Festinger, Professor of Psychology, and Henry W. Riecken (seminar chairman), Associate Professor of Sociology, University of Minnesota; and Gilbert K. Krulee, Research Assistant Professor, Department of Systems Analysis, Tufts College. They devoted most of their efforts during the seminar period to designing specific research projects in which a "field" finding will be tested in a laboratory situation or vice versa. This statement, which was prepared as a by-product of the seminar discussions rather than as a report on the main objective of the seminar, is an attempt to generalize the problems encountered in making the transition from field to laboratory, in specific empirical studies.

In conducting an experiment the social psychologist ordinarily tries to control the personal characteristics of the subjects, the environmental influences to which they are exposed, their activities, and some of the events that occur during observation, i.e., the treatment or manipulation intended to produce certain behavioral consequences. Experimental control is never perfect, of course, and some conceivably important variables are allowed to fluctuate spontaneously, usually with the expectation that their effects will be randomly distributed.

In a field study, on the other hand, the investigator finds subjects who are engaging spontaneously in activities of interest to him; their activities are an ordinary, natural consequence of their life situation and are not dependent on his desire to study them. Furthermore, the field worker usually observes the events that occur during the period of study but does not attempt to manipulate the experiences of subjects or to produce particular effects. To be sure, he may choose to observe a situation in which he expects the subjects to encounter particular events or experiences, as when he studies the effects of a criminal rehabilitation program, a public housing project, or an information campaign. But he does not determine the nature of the experiences and events. If the research worker is called into program planning or allowed to stipulate some of the events or experiences of subjects, while the majority of control still resides in the hands of some other agency (including the subjects themselves), we have a third variant of social psychological studies, namely the "field experiment."

The distinction drawn here between field and laboratory research cuts across the common distinction based

on the place where the research is conducted. Thus if a family were brought to an observation room and set up housekeeping there while psychologists and sociologists observed them through a one-way screen, we would call this a field study and find it no different, *in principle*, from posting observers in the family's home. Similarly if an experimenter were allowed to impose a schedule of activities on a family in its home, and varied systematically the amount of communication among family members while he tested its effects on children's identification with parents, this would be a laboratory study. Experiments generally take place in university laboratories with college students as subjects, while field studies go on outside the university, but this fact is less important, in principle, than the difference in control.

The role of theory. There are probably two main reasons for deciding to study a particular problem: its theoretical significance, or its social or practical importance.

It is our impression that in the majority of instances research undertaken to test a hypothesis derived from theory, from findings previously obtained, or from conclusions reported by other investigators is likely to take the form of a laboratory experiment. The chief reason is the nature of contemporary theory in social psychology. It is usually not internally complex, but is highly abstract or general, and capable of dealing with only a few variables at a time. Thus the testing of such theories, or deductions from them, demands severe restriction of investigative attention. Because social psychologists know so little about the relative importance of various factors that may influence a particular behavior, their safest procedure in studying the effects of the few variables their theory can handle explicitly is to eliminate, hold constant, or randomize the operation of additional factors which would ordinarily co-vary in natural situations. Since it is difficult, if not impossible, to find a natural situation simple and restricted enough to conform to the demands of an elementary theory, the social psychologist is virtually forced to contrive a situation that will give his hypothesis a fair test.

On the other hand, it is our impression that most field studies originate from the desire to understand some problem of social or practical importance. Typically, then, a field study is concerned with a situation that is independent of the investigator's curiosity. Since he wants to study what is, not what might be, he is not interested in introducing controls or restrictions: these would only alter or destroy the very thing he is trying to study. The natural situation is the center of attention, and theoretical concerns are usually secondary. Often the primary aim of the field study is to accumulate em-

pirical facts; theory enters the picture only after they are collected and serves simply to codify them. Sometimes theory plays more than a classificatory role, and statements of relationship among empirical findings are derived by induction. Even in the latter case theoretical statements emerging from field studies seem to be generally at a lower level of abstraction than those from laboratory experiments and likely to be more closely connected with specific empirical events.

Without making any judgment as to the preferable level of abstraction, it may be noted that this different orientation toward theory results in noncorrespondence between field and laboratory findings. Here is the principal gap between the two kinds of studies. Since experiments tend to be concerned with theoretically generated relationships which are relatively independent of the natural world, while field studies focus on concrete relationships that are interesting in themselves but not necessarily relevant to a general theory of social behavior, conclusions reached through the two methods will not necessarily agree nor even be related. Field workers tend to talk a different dialect from that used in laboratories, and field and laboratory results are less often contradictory than incommensurable.

Conclusions from a laboratory experiment are likely to be stated so abstractly that they have only the most general relevance to any specific natural situation. On the other hand, conclusions from field studies are likely to be so specific, conditional, and internally complex that they are not easily manipulated conceptually. This difference in the degree of abstraction arises from a parallel difference in the situations scrutinized. Experiments are usually done in situations that are both phenomenally and conceptually as simple as possible, and experimenters proceed by manipulating elementary variables in particular combinations that are considered important by theoretical standards, without regard to the likelihood of their occurrence in exactly this form in natural situations. Every effort is made to have the test and the resulting conclusion as rigorous as possible. In contrast, most field studies seem to begin with naturally occurring combinations of many variables, accepted in their full complexity, and proceed to explore the range of combinations spontaneously occurring in the situation at hand. The intention is to arrive at conclusions as comprehensive as possible.

"Realism" versus "artificiality." This difference between simplicity and rigor, on the one hand, and complexity and comprehensiveness, on the other, often masquerades as an antithesis between "realism" and "artificiality." We consider this antithesis misleading. Subjects of field studies are responding to "real" forces

in the sense that these forces would impinge upon them and (probably) affect them in the same way even if no investigation were in progress. Laboratory subjects under synthetic conditions may be responding to forces they have never encountered before and may never meet again. But if we believe that "what is real is what has effects," how can the situation of laboratory subjects be less "real" than that of subjects in a field study?

Two questions of great importance and complexity seem to be at issue here: (1) To what extent can the findings of any investigation be generalized? (2) To what extent do phenomenal events correspond to the conceptualization of them?

It would not be profitable to discuss the second question here. Whether one begins with observation and tries to conceptualize it, or begins with a concept and attempts to find a situation that embodies it, one faces the problem of correctly coordinating phenomena with concepts. The problem takes different form in the two cases but is essentially a general problem in scientific work, without special relevance to the differences between field and laboratory research.

Generalization. The question of generalization of results, however, has distinct relevance to our present topic. Either laboratory or field research can yield ungeneralizable results, or conclusions whose limitations cannot be accurately determined, but for somewhat different reasons. In field studies the situation being studied may be too complex to permit determination of which, or what combination, of the many variables are significantly, rather than accidentally, related to the effects observed. In laboratory experiments, where deliberate manipulation of selected variables is possible, results may not be generalizable because the design did not take into account the effect of certain variables which were randomized or held constant, but which would have overshadowed the effect of the variables that were manipulated, had the former been allowed to act. Laboratory experiments often suffer from involuntary restriction in the variables that can be observed because of the rather rigid circumstances in which they usually take place—e.g., the social context of the university and the more or less well-defined interpersonal relationship of student and teacher, which tends to restrict the roles that subject and experimenter can play and probably limits the attitudes and expectations, not to mention the reactions, of the subjects.

To what end should results be generalizable? If the aim of research is to predict the behavior of specific people in specific circumstances, then the more closely the situation studied corresponds to the situation to be predicted, the more successful the effort should be. Thus

for testing specific hypotheses, stated in concrete terms, a field situation that closely corresponds to the one in which the hypothesis was generated probably offers certain advantages. If, on the other hand, the aim is rather abstract formulations of relationship among theoretical variables which may take many concrete forms, it seems unlikely that studying complex concrete situations is the most expeditious route to the goal.

Practicality of research. This last point has some bearing on the often argued question of "practicality" or usefulness of field and laboratory research. It can be argued that an abstract formulation of relationships may suggest new, and phenomenally unrelated, connections in the empirical world. A good theory, with its capacity to generalize across concrete phenomena, can embrace a great variety of empirical observations and suggest new experiments or studies to fill gaps in present knowledge. In this sense, then, a good theory is a practical device; and laboratory experiments, too, are practical to the extent that theories are developed through them. But practicality in this sense is a long-range proposition, requiring many interrelated experiments which individually may yield few directly applicable results. Furthermore, many of the applications of a developed theory are quite irrelevant to the substantive questions that were important in initiating the inquiry. There is no guarantee that an investigator who sets out to develop a theory from a particular kind of concrete event will emerge from his preoccupation at a point even remotely connected with the phenomena from which he began. If specific substantive knowledge is the aim, field studies rather than experiments seem to be indicated.

This view does not imply that field studies have merely an "engineering" role in the development of behavioral science. Field studies are most useful in suggesting new ideas, new relationships, and, indeed, new theories. Whether knowledge from field studies is as readily cumulative as that from laboratory experiments is debatable, but it is clear that a behavioral science cut off from contact with nature could rapidly deteriorate into sterile play with abstractions. All research in behavioral science is grounded in observations of nature, and the more comprehensive our observations can be, the more powerful our theoretical constructions will be.

Correspondingly, it is clear that laboratory experiments can be so narrowly conceived or so strictly controlled that new information is precluded, and the experimenter can succeed merely in cutting himself off from observations that would lead to modification and development of his theory. One of the best ways to shelter a theory of social behavior is to segregate it from

contact with everyday facts. It does seem reasonable to ask that any behavior theory cope with some of the complexities of natural situations, even though these complexities may be beyond the present capacity of the theory, for only by such confrontation will its weaknesses be revealed and the opportunity offered for its extension and refinement.

Replication. In concluding this comparison of field study and experimental contributions to a cumulative knowledge of social behavior, we feel bound to mention an important defense against error that is more readily available to the experimenter than to the field worker, namely replication and controlled variation. Most experiments, if they are adequately reported, can be exactly replicated. If an exact replication produces different results, it is clear that the experimenters have failed to control important and relevant variables. If partial variation of an experiment produces unpredicted results or results inconsistent with earlier findings, it is probable (barring errors of measurement) that important variables have been ignored or that operations and theoretical constructs have not been well coordinated. Of course, if repeated replication produces unpredicted results, then the theory may be wrong. The important point is that it is possible to replicate experiments but it is rarely, if ever, possible to replicate field studies. Thus experimental findings can be subjected to the scrutiny of other investigators and, within limits, questions can be resolved; in many field studies, however, questions or objections must remain in the realm of conjecture.

In brief, then, the gap between field studies and laboratory experiments is one of incommensurable results. This gap comes from a different orientation toward the role of theory and toward the importance of the natural situation; from the somewhat different purposes motivating field workers and experimenters; and from the difference in their interest in empirical facts and abstractions.

Inherent divisive tendencies. Not only do field studies and experiments start from different points and proceed in somewhat different directions, but forces inherent in the research situations tend to pull them farther apart. Once one becomes immersed in a natural situation, one often becomes intrigued with the facts of the case and tends to pursue their ramifications even when they lead away from the question that initiated the research. It is perhaps as much through this kind of natural curiosity as anything that field workers tend to become involved in concrete rather than abstract or theoretical questions. Furthermore, a certain measure

of devotion to the practical problems of a group or organization may help the investigator to gain and maintain rapport with his subjects. On the other hand, for the experimenter, there is fascination in sheer intellectual manipulation of concepts and the creation of a rigorous, esthetically gratifying design. It may become more important to produce neat results than to work on problems of relevance for a general, workable theory of social behavior. These diverting and divisive tendencies are present in all field and laboratory work, but they need not be our masters.

So far we have tended to build two straw men—"the experimenter" and "the field man"—and to stereotype their activities and interests. There is scarcely a statement in these pages that cannot be challenged by at least one counterexample and occasionally by several. But our intention has been to identify and illuminate the forces that have produced the gap between field and laboratory. We may now mention some of the trends toward closing that gap.

RECENT TRENDS IN SOCIAL PSYCHOLOGICAL RESEARCH

Over the past two decades there has been an increasing use of theory in all of social psychology. Purely descriptive studies have decreased in number and importance, and there has been a notable tendency for field investigators to test specific hypotheses through systematic observation. Above all, there has been a tendency to select situations for observation because of their relevance to general or theoretical problems, rather than simply because of their availability. In experimental work the same trend is seen in the tendency to treat variables that have a systematic status in psychological theory rather than to manipulate mechanical or superficial factors without regard to their psychological significance. Fewer studies are being made "to see what would happen if . . ." and more to test deductions from theory.

Second, we note the gradual growth of interest in field experiments, where a natural situation is partially controlled. Such experiments have been conducted in factories, housing projects, military organizations, reformatories, boys' camps, clubs, hospitals, construction gangs, girls' schools, kindergartens, and in other sites outside university laboratories.

These trends are encouraging, even though they but point the way toward narrowing the gap between field and laboratory. Most of the "gap-narrowing" research known to members of the seminar has been motivated either by a desire to use social psychological knowledge to improve the efficiency of practical operations, or by

an interest in testing hypotheses derived from one set of conditions under somewhat different conditions. In the latter case the investigator usually has wanted either to test his ideas more rigorously in order to introduce greater refinement and precision into his theoretical scheme, or to test its power by confronting it with a more complex situation where variables that had been involuntarily held constant could be allowed to operate.

From this last observation we would conclude that the gap is likely to be narrowed through research on theoretical or practical problems undertaken with either of the two motives mentioned, rather than by self-conscious organized attempts to fill the gap as such. In short, the differences between field and laboratory research will be reduced as a by-product of research effort rather than directly.

In moving from field study to the laboratory, or vice versa, the investigator faces a variety of problems, some common to all scientific effort and some specific to this transition. The latter are greater when the movement is from laboratory to field, and they begin with the elementary problem of finding a suitable natural situation for study. This is largely a matter of knowing enough about the world to locate an appropriate situation, gaining entrance to it, arranging for observations, measurements, and, in a field experiment, manipulations. These are difficult but not insurmountable problems, for many investigators have utilized natural situations well. Laboratory workers can probably learn much about how to overcome resistance to research in organizations from their colleagues in labor relations and industrial management research. It is also true that personal skill in persuasion, sensitivity to the needs and feelings of others, and a willingness to work on "engineering" problems as well as on one's own research can help in securing opportunities for field research. There are almost sure to be technical problems of design, e.g., keeping the treatment rigorous, preventing contamination of controls, and getting enough cases in all categories. There will also be problems of procedure, e.g., concealing the purpose of research, where this is necessary in order to avoid influencing the outcome, without at the same time leaving the subjects baffled and suspicious. A most important technical problem is that of developing appropriate measuring devices and manipulations for field situations. Many current laboratory procedures for controlling or analyzing communication, establishing an attitude toward a task, or creating a particular affective relationship among subjects are impractical or impossible in field situations. Similarly, most techniques of observation and measurement are adapted to small spaces, limited numbers of contacts or communications, and restricted activities

of subjects. Hence they are of limited use in natural situations.

CONCLUSIONS

Our survey has convinced us that the gap between field studies and laboratory experiments is narrowing rather than widening, largely because of the increasing penetration of theoretical concerns throughout social psychology, and through such methodological devices as the field experiment. We think this trend toward better integration is progressing spontaneously, although there may be some deliberate policies that can further the trend. We are, of course, completely opposed to the regimentation of scientific efforts, not only on moral but also on pragmatic grounds. There is no one best way to collect data, and it would be unreasonable to insist that all social psychological research should be of the "gap-narrowing" variety. Differences in temperament, experience, and training make some investigators more suited for laboratory work, others for field studies. Furthermore, too great concern with rigor and control can dampen creative insights, gained from free observation of natural situations, while too keen a desire to test a new theory in a complex situation can lead to the hasty conclusion that it is incorrect when, in fact, it is merely inadequate.

These conclusions are not to be construed, however, as satisfaction with the status quo. We believe that the desirable trends can be encouraged in specific ways:

First, there is need for more communication between laboratory and field workers. This cannot be achieved simply by attaching a "field man" to a laboratory study nor by stationing an experimenter in a field project. Useful communication is possible, however, when these two kinds of specialists can consult each other about ideas or plans. A competent investigator knows approximately what he wants to find out, but may not be aware of all the possible ways of going about his research or of the difficulties or problems in a particular technique or situation. On the other hand, an investigator may need what amounts to instruction rather than advice when he attempts a technique outside his special skill. When a field man is motivated to undertake a laboratory experiment, or an experimenter wishes to study a natural situation, any policy or program to increase his opportunity to communicate with a sympathetic opposite specialist would be useful. On the other hand, a policy aimed at *creating* this motivation would not, in our view, be as desirable.

Second, there is need for more powerful and more embracing predictive theory in social psychology. The development of such theory cannot be accomplished

willfully or by fiat, but it can be nourished. We have pointed out that theoretical developments come from two sources: an intimate connection with nature, which gives theory relevance to the real world and provides the complexities and confrontations needed for innovation; and a firm basis in experimental tests under controlled conditions. Encouragement and support of laboratory or field investigations that have distinct theoretical relevance will help to narrow the gap. This point applies with even greater force to any research plan that, besides having theoretical relevance, involves a field test of an experimental finding or an experimental test of a field finding.

In going from field to laboratory, or vice versa, it is not always desirable to demonstrate phenomenal similarity between the situations being studied; in fact, such "face validity" may actually be restrictive. One usually learns more from cross-testing in situations that are *phenomenally different but conceptually parallel*. An exact phenomenal replication, in the laboratory, of a field situation tends to yield minimum information, but the greater the phenomenal difference, the more one can learn about the power and generality of his theory.

Third, more information is needed about natural situations that are available for investigation. It is often difficult for an experimenter who wishes to test some hypothesis in relatively uncontrolled situations to know where to go in order to find an appropriate one, or even of whom to make inquiries. There is need for some efficient channels of information concerning ordinary, accessible events and situations that will meet the requirements of the theory being scrutinized. Precisely how such information could be assembled and made accessible is anything but clear. Whatever the specific means chosen, any way of broadening the investigator's familiarity with his social environment will be helpful in narrowing the gap between field study and laboratory experiment.

Fourth, there is need for invention and development in measurement techniques. Theoretical concepts can

often be empirically realized under rather special circumstances in laboratory experiments, but there may be no corresponding measurement technique that is practical in a field situation. For example, laboratory experiments on communication problems often have subjects write notes to each other. This convenient way of measuring frequency, target, and content of communication is wholly unsuited to study of an industrial shop or infantry squad, of course. One unfortunate result of the lack of appropriate instruments is that the research worker sometimes measures what he can with existing instruments rather than what he really wants to measure. This need for better techniques is present in laboratory experiments, too, but is perhaps greater in field studies.

Fifth, in addition to bringing the laboratory into the field by means of field experiments, it would be profitable to bring the field into the laboratory, especially by using persons other than college students as subjects and by trying to arouse motivations other than curiosity, deference, and the desire for academic credits or for money paid for "acting as a subject." Some attention might well be paid to varying the interpersonal relationship between subject and experimenter, as well as the institutional context in which they interact. Such changes might reveal the effects of variables that are involuntarily held constant in the traditional form and locus of laboratory experiments. Just what new insights might be gained or what variations would be most profitable is not known, but self-conscious scrutiny of the established patterns of laboratory research might suggest better strategies.

Finally, we concur in the recommendation frequently made by others that replications of experiments be strongly encouraged. Replication and systematic variation of experiments are perhaps the best defense against erroneous conclusions and overgeneralization, and one of the most useful means of expanding theory and making it more adequate for explaining the complexities of social behavior in natural situations.

OCCUPATIONAL CHOICE: A BRIEF REPORT ON THE 1954 SEMINAR

by Herbert S. Parnes

THE interuniversity summer research seminar on occupational choice was held at Ohio State University from June 21 to August 13, 1954. The participants were: Peter M. Blau, Assistant Professor of Sociology, University of Chicago; John W. Gustad, Director, University Counseling Center, University of Maryland; Rich-

ard Jessor, Assistant Professor of Psychology, University of Colorado; Herbert S. Parnes, Associate Professor of Economics, Ohio State University; Leonard Reissman, Assistant Professor of Sociology, Tulane University; and Richard C. Wilcock, Assistant Professor of Labor and Industrial Relations, University of Illinois.

Although none of the participants had done research specifically on occupational choice, each has research interests closely related to that topic. Two are especially interested in the sociological implications of vertical occupational mobility. Two have been engaged in labor market research and have been impressed by the need for additional information on the factors that motivate workers' labor market behavior. One is currently preparing a survey of the literature on vocational interests; and the special interest of another is in a social learning approach to personality and in the relation between hierarchies of psychological needs of individuals and occupational choice. Thus, the participants came with widely different backgrounds but with a common interest in the factors that determine occupational choice. Prior to meeting in Columbus each had attempted to think through the content of the seminar individually and had prepared a statement of his specific interests in occupational choice and his suggestions relative to the objectives of the seminar. In addition, some submitted bibliographies, and the volumes included were assembled and reserved for seminar use.

From the very outset there was consensus that the seminar would make a valuable contribution if it could do three things: (1) develop an interdisciplinary research approach to the problem of occupational choice, (2) construct a schema for research on the subject, and (3) suggest specific areas of research. The seminar met for three hours each morning and reserved the afternoons for individual study and writing. There appeared to be a preference for as little group organization as possible, and the seminar functioned during the entire eight weeks without a formal chairman and without a secretary. Each participant kept his own notes.

The first week and a half was devoted to discussion of the significance of the problem of occupational choice to each of the three disciplines represented at the seminar and to the contributions that each of these disciplines had made or might make to the study of occupational choice. This introduction proved to be very helpful, for during this period most of the problems of communication growing out of the interdisciplinary nature of the seminar seemed to disappear. Also the participants became well acquainted as individuals: they began to be aware of and to understand each other's personal idiosyncrasies, biases, and preferences—factors that can create as many problems in joint research as do differences among disciplines. During the remainder of the second week no group meetings were held. This period was devoted exclusively to individual study.

For the next several weeks, discussion centered on the nature of the problem of occupational choice, the nature of an interdisciplinary approach to the problem,

a schema for research on occupational choice, and specific areas of research. According to the general procedure, each participant prepared a statement on each of these four topics, and these were then subjected to intensive discussion and criticism during the group meetings. Out of these discussions there generally emerged a joint statement or formulation which represented, more or less, the consensus of the group. The final two weeks of the seminar were devoted to preparation of a detailed outline of a proposed monograph on occupational choice, which would present the conclusions reached by the group. Each member of the seminar has assumed responsibility for writing portions of this monograph; the first drafts are to be submitted to the writer by January 1, 1955.

Space does not permit even a brief resumé here of the substance of the eight weeks of seminar work, but some of the problems encountered and the frame of reference selected by the seminar may be described. There are several points of view from which occupational choice can be studied. One can be concerned with the factors that explain the size and composition of the labor force and its distribution among occupations. One can investigate the social and economic implications of a given occupational distribution and its effects on wage structure, social stratification, etc. Or one can study the determinants of the labor market behavior of the individual, that is, attempt to explain the processes whereby individuals enter particular occupations or jobs.

The difficulty, apparent at the very outset of the seminar, is that these alternative frameworks are of varying degrees of interest to the psychologist, the sociologist, and the economist. The psychologist is concerned with occupational choice as a specific manifestation of individual behavior, and with the interrelation between occupational choice and the personality structure of the individual. On the other hand, neither the economist nor the sociologist is interested in the behavior of the individual *per se*. The sociologist's concern with occupational choice is generally its interrelationship with social stratification. The economist views occupational choice in relation to the allocation of human resources in the economy. However, any theory of social stratification or of resource allocation depends in the last analysis on certain assumptions with respect to the way *individuals* behave. The explanatory or predictive efficiency of the theory can be no better than the validity of these basic assumptions. From this point of view a theory of occupational choice that explains how individuals are channeled into occupations is no less important to the sociologist and the economist than to the psychologist. It was consequently agreed that the devel-

opment of an interdisciplinary approach to such a theory was a legitimate objective of the seminar, irrespective of the uses to which the theory might possibly be put by members of the individual disciplines.

The second question that had to be faced was whether the seminar should be concerned with the consequences as well as the causes of occupational choice. It is obvious that, within the framework of each of the disciplines represented, occupational choice is both a dependent and an independent variable. For example, although psychologists regard occupational choice as some function of the personality structure of the individual and of his environment, they recognize that the occupational distribution of the labor force is among the significant environmental factors to which the individual responds, and that the specific occupation of the individual is presumably one of the factors that condition his personality. Sociologists, while stressing the importance of social class as a determinant of occupational choice, at the same time view the occupational structure as a factor affecting class hierarchy. Economists traditionally focus on the wage variable in explaining the movement of workers into and among occupations, but also are concerned with the effect of the movement of workers on the wage structure. In view of the complexity of the subject and of the time limitation, it was agreed that the seminar would not attempt to develop a research framework broad enough to include the economic and social consequences of the occupational choices of individuals, although it was recognized that these have important implications for economics and sociology.

Finally, within the framework described above, the term "occupational choice" itself posed certain difficulties. It is perhaps unfortunate that the terms "occupational choice" and "vocational choice" have become so firmly fixed in the literature relating to the processes whereby individuals enter occupations or pursue careers. The word "choice" has connotations which may be completely irrelevant or even misleading in this context. It may, for example, create the image of a completely free agent making a single decision on the basis of some hedonistic calculation which automatically results in his entry into a particular line of work. There are at least three objections to the use of the term "choice" in this sense. First, it is obvious that the decision to enter an occupation or to take a particular job is really the product of many prior decisions by the individual—whether to watch television or to read during adolescence, whether to take a commercial or a college preparatory course in high school, whether to go to college, etc. Thus occupational choice cannot realistically be conceived as the result of a single deci-

sion. Second, an individual is never a free agent with respect to any of these decisions in the sense of being unaffected by factors external to himself. For example, the son of a wealthy investment banker is not ordinarily "free" to decide to become an automobile mechanic, either because the values that have been instilled in him make it unlikely that such a decision will even occur to him or because, if it does, pressure of family and friends will successfully intervene. Finally, regardless of the decisions that individuals make, their intentions may be thwarted by factors quite external to themselves, such as the high cost of a college education, racial discrimination, or a plant shutdown.

On the basis of these considerations it was decided that occupational choice realistically refers to the entire developmental process that leads an individual into a particular occupation. This process represents a complex interaction between the individual and his environment over an extended period of time. In this sense the term "occupational choice" is no less applicable to the individual who has been buffeted about by his environment all his life, than to the individual who has successfully manipulated the environment in order to achieve goals that were firmly established early in life. During his pre-career development as well as during his participation in the labor force, an individual does make "choices" when he behaves in one way or another. Thus entrance into high school represents a choice to continue one's education. Selection of a curriculum in college is a choice, even though it may be made on the basis of a flip of a coin, the advice of a friend, or the insistence of a parent. Applying for a particular job, even if it is the only one available in the locality, is a choice in that the individual might have "chosen" to go on relief or to move to another community.

As distinguished from "choices," which realistically may be almost exclusively the result of environmental factors, the seminar used the term "preference" to refer to the individual's subjective evaluations of alternative courses of action, or of alternative jobs or occupations. These preferences may be realistic or unrealistic; they may have been arrived at rationally or irrationally. It is implied only that individuals do have some kind of ranking of the alternatives of which they are aware.

If occupational choice is conceived as a developmental process that culminates in the entrance of the individual into an occupation, it is pertinent to ask to what extent individuals are actually attached to occupations more or less permanently. It is clear that there is a high degree of stability of membership in the professional occupations, but it is not known to what extent manual workers and subprofessional white collar workers have similar career orientations. (It is hardly a

coincidence that most of the studies of occupational or vocational choice have been made with respect to professional occupations.) It may be that the kind of occupational attachment characteristic of a professional worker (e.g., an architect) exists among manual workers only in terms of broad occupational groups (e.g., machine operators). If this should be the case, predictions of occupational choice would have to be made with varying degrees of specificity. For example, an occupational classification system in which *architect* is coordinate with *machine operator* might be required. Thus the definition of "occupation" in a general theory of occupational choice cannot be made in advance but

must be made on the basis of empirical research. The classification of occupations into groups that are homogeneous with respect to the stability of their membership would be a useful research project.

As a final word in the way of evaluating the summer seminar, it was a richly rewarding personal experience for each of the members. The opportunity to think through leisurely a complex problem together with able representatives of related disciplines would not exist in the absence of a program such as that sponsored by the Council. It is hoped, of course, that the seminar will have wider values as well as those that have accrued to the participants as individuals.

AMERICA'S RESOURCES OF SPECIALIZED TALENT

by Dael Wolfe

THE title of this note is also the title of the final report of the Commission on Human Resources and Advanced Training, which was published by Harper & Brothers in September 1954.¹ The Commission's task was that of determining how the nation utilizes its intellectual resources. In a period of increasing attention to manpower problems, the Commission studied the supply of persons in the natural and social sciences, the humanities, the professions, and the other higher-level fields of specialization. The studies covered not only questions of supply but also consideration of the growing demands for various kinds of specialists, and analyses of the potential supply of new recruits to the specialized fields which can be expected in the years ahead.

There are two rather fundamentally different ways of studying the nation's specialized manpower and its utilization: analysis in terms of supply and demand; analysis of the extent to which we are utilizing our intellectual resources. Both methods were used.

With regard to supply and demand, the Commission compiled and developed several kinds of information. The gradual shifts in emphasis among different fields of specialization in college have been traced since the beginning of the century. For example, at that time less than 4 percent of the bachelor's degrees conferred in the United States were granted to students who had specialized in one of the social sciences. Since 1951 some

11 percent of bachelor's and first professional degrees have gone to such students. In the same time span, degrees to students in the humanities and arts have dropped from 25 percent to 12 percent of the total. Neither of these shifts, however, has been as large as the tremendous growth of professional schools of education and of business and commerce. Both have grown from being very minor contributors to the total number of degrees to the point where they now exceed all other fields of collegiate concentration.

Another study followed the postcollege careers of a sample of the nation's college graduates to determine the occupational distribution of those who had majored in each field. Students who had majored in medicine, dentistry, law, and engineering typically followed careers in those fields. In contrast, men and women who had taken undergraduate majors in the liberal arts were less likely to follow careers in those fields than in something else. In social science, for example, 5 percent of the graduates were estimated to be working as social scientists; 18 percent had entered other professional fields; 12 percent were teaching in elementary or secondary schools; 21 percent were in business; 10 percent were in administrative positions other than those in business or elementary or secondary schools; 15 percent were employed in nonprofessional work; 10 percent were engaged in professional or graduate study; and 6 percent were not in the civilian labor force. Such figures for the total group of living graduates and comparable ones for students who earned degrees in 1951 provided a basis for estimating the occupational distribution of college graduates of the next few years.

The most difficult problem in projecting the labor

¹ The Commission was appointed by the Conference Board of Associated Research Councils in 1949 and was terminated soon after publication of its report. Its members were: Charles Odegaard (chairman), M. H. Trytten (vice-chairman), Donald Bridgman, Aaron J. Brumbaugh, C. W. de Kiewiet, Ovid W. Eshbach, E. D. Grizzell, Quinn McNemar, Ralph A. Sawyer, Frederick F. Stephan, Paul Webbink, Malcolm M. Willey; staff, Dael Wolfe, Director.

market in the specialized fields is the projection of future demands, for which an adequate methodology has not yet been developed. In lieu of better methods the Commission traced the past employment trends and collected estimates and projections from a variety of sources to provide rough guides concerning the future demand in each field.

For long-range planning it may be less useful to analyze supply and demand factors than to consider the extent to which the nation utilizes its intellectual resources and the extent to which utilization might be improved in the future. That many young men and women who are potentially qualified for work in the professions and other specialized fields do not secure the advanced education necessary for entry to those fields has been widely known and was thoroughly documented in one of the Commission's studies. To secure better information on the occupational records of such persons, the Commission studied three samples of superior high school graduates of approximately 20 years ago. Those selected for study had either ranked well enough in high school grades, or had scored high enough on tests of academic aptitude, to indicate that they would have been average or better college entrants. Questionnaires were used to determine what education each had had beyond high school, what position he held in 1953, and the size of his annual income.

It has frequently been reported that college graduates earn more than high school graduates, and that high school graduates earn more than those who did not attend high school. The Commission attempted to go beyond such information to determine the earnings and positions occupied by persons who were comparable in ability but who differed in amount of education, and similarly to determine the earnings and positions of persons who had the same amount of education but who differed in the ability demonstrated in high school.

It turned out that both ability and education were clearly reflected in the positions occupied and incomes received by these high school graduates of some 20 years ago. Of those with the same amount of education, the ones who had been brighter or better students in high school occupied higher positions and received higher incomes in 1953 than those who had done less well in high school. Of those who appeared to be equal in ability at the time of high school graduation, the ones with the most education were occupying better positions and earning higher incomes than those with less education. The margin of advantage was greater for differences in amount of education than for differences in ability within the restricted range of ability studied. Within any given educational group, those who had looked most promising during their high school

years were earning a few hundred dollars a year more than those who fell closer to the average of their high school classes. In contrast, among those within the same ability group, the ones who had graduated from college were earning from \$1,000 to \$2,000 more annually than were their high school classmates who had not entered college. Significantly, the advantage of college graduates in income and position was greatest for those of highest ability. High school graduates whose ability was slightly above average profited from going to college; the very superior high school graduate profited even more.

This high school study and related work by the Commission indicated clearly that the nation has not been tapping anything like all of its intellectual resources. Each year many able high school graduates choose not to enter college. The reason is sometimes financial; frequently—perhaps more frequently—the explanation lies in the attitudes toward education, the greater attractiveness of other courses of action, and the social and cultural milieu in which the boy or girl grew up.

In partial compensation for our failure to use all the nation's intellectual resources is the great flexibility with which the well-educated fraction of the labor force is employed. That college graduates who had majored in most of the fields of college specialization were later found to be employed in a wide variety of fields of professional work has been mentioned. A considerable portion of this occupational mobility represents interests, experiences, and demands that changed after formal education was completed. Some of the best data on this point pertain to natural scientists. For example, 37 percent of the men with Ph.D. degrees who were listed as physicists in the 1948 edition of *American Men of Science* had had previous experience in biology, chemistry, engineering, or some other broad field of science. The upsurging demand for physicists since the beginning of World War II converted a substantial number of biologists, chemists, engineers, and mathematicians into physicists, and even recruited a few from more remote fields. The staffing of electronic and nuclear energy projects dramatized the occupational flexibility of highly qualified specialists, but the same process goes on continuously. The flexibility of the nation's educated specialists is a real asset in meeting changing demands.

The Commission's studies point to several ways in which better utilization of the nation's intellectual resources could be achieved. First, of course, is the identification of youth of highest potential. After the most promising are identified, appropriate education is required. With the demand for educated specialists steadily growing, that means that college training is usually required. Yet many who could profit from col-

lege training do not get it. The granting of college scholarships is an obvious means of helping to increase the number who do, but that solution can be only partially effective. Many persons who might have grown into distinguished scholars or professional men and women were so completely sidetracked into other areas that a scholarship offer would not have induced them to change their plans. Full utilization of ability demands not only the money with which to pay for advanced education but the wish for that education and the habits of thought and application that would make additional years in school profitable. To increase the wish for education on the part of those who could profit most from it is a social and educational problem more difficult to solve than the financial one.

The character of the education to be offered is also

important. As elementary and secondary education grows more and more nearly universal, the problem of giving the best preparatory education to the minority of students who will become scientists, humanists, and members of other professional areas grows in complexity. As students go through high school, enter college, select the fields in which they specialize, and then spread out into a variety of fields of work, the question of what kind of education can best serve their needs persistently arises. It was never the Commission's purpose to work directly on secondary school or collegiate curricula, but that problem is inescapable. The nation's supply of specialized talent is in large part determined by how the talent is educated; America's future resources of specialized talent will depend primarily upon what the nation's schools accomplish.

COMMITTEE BRIEFS

CROSS-CULTURAL EDUCATION

Ralph L. Beals (chairman), Cora Du Bois, Herbert Hyman, Ronald Lippitt, Charles P. Loomis; *staff*, Joseph B. Casagrande, M. Brewster Smith, Bryce Wood.

Principal investigators from the four projects comprising the committee's program for 1954-55 met at the University of Wisconsin for a six-week workshop under the chairmanship of M. Brewster Smith during June and July. Participating for the entire period were: John R. P. French, Jr. and Robert B. Zajonc of the University of Michigan, who are engaged in an experimental study of how conflicts between cultural norms are resolved; Rose K. Goldsen of Cornell University, who is studying the values of foreign students and their close American associates, as related to patterns of intergroup association and to changing attitudes and beliefs; Richard T. Morris of the University of California at Los Angeles, who is studying differing perceptions of national status as a factor in foreign students' attitudes and adjustment; and Claire Sellitz of New York University, who is investigating the effects of foreign study in different kinds of institutional settings. Stuart W. Cook of New York University and Robin M. Williams, Jr. of Cornell each spent one week with the workshop.

To promote continuity with previous research in the field, a resource file of unpublished research was made available to the group, and participants in the committee's previous research were brought in as consultants. The workshop was thus able to draw on the experience of John W. Bennett of Ohio State University, Oluf Davidsen formerly of the University of Wisconsin, and Richard D. Lambert of the University of Pennsylvania in their respective studies of Japanese, Scandinavian, and Indian students.

The hypotheses and plans of each of the 1954-55 projects were critically discussed at the workshop. As the interrelations of the studies became explicit, the group in joint sessions attempted to plan a common treatment of concepts linking the studies. A minimum common core of background factors and of dependent or "outcome" variables was agreed upon. The central hypotheses of the projects led to concentration on effects on the foreign student's attitudes toward the host country. While collection of data in the four studies is only beginning, participants in the workshop were convinced that it had not only benefited their individual projects but had also achieved a degree of integration not otherwise attainable among them.

The final reports of the initial studies undertaken by the committee are being prepared for publication. Each of these studies is represented by an essay in "America through Foreign Eyes," the September 1954 issue of the *Annals of The American Academy of Political and Social Science*, edited by Richard D. Lambert.

ECONOMIC GROWTH

Simon Kuznets (chairman), Richard Hartshorne, Edgar M. Hoover, Bert F. Hoselitz, Wilbert E. Moore, Morris E. Opler, Joseph J. Spengler.

The papers prepared for the conference on the role of cities in economic development and cultural change, held last May under joint sponsorship of the committee and the University of Chicago, appear in the October and December issues of *Economic Development and Cultural Change*. Included are "Urbanization and the Development of Pre-Industrial Areas" by Kingsley Davis and Hilda H.

Golden of Columbia University; "The Social Structure and Function of Cities" by William L. Kolb of Tulane University; "The Cultural Role of Cities" by Robert Redfield and Milton B. Singer of the University of Chicago; "The History of Cities in the Economically Advanced Areas" by Eric E. Lampard of the University of Pennsylvania; "A Description of Certain Spatial Aspects of an Economic System" by Rutledge Vining of the University of Virginia; a summary entitled "Generative and Parasitic Cities" by Bert F. Hoselitz of the University of Chicago; and comments by other conference participants.

Two further conferences in the series organized by the Committee on Economic Growth have been held this autumn. A conference on investment criteria for economic growth, sponsored jointly with the Massachusetts Institute of Technology Center for International Studies, was held at Cambridge in October. The papers discussed at this conference included "The Relevance of Theoretical Criteria in the Selection of Investment Plans" by Jan Tinbergen of the Central Planning Bureau of the Netherlands; "Programming in Theory and in Italian Practice" by Paul Rosenstein-Rodan of Massachusetts Institute of Technology; "The Allocation of Investment in Underdeveloped Countries: The Case of Burma" by Everett E. Hagen of Massachusetts Institute of Technology; "Economics and Investment Planning—Some Reflections Based on Experience in Colombia" by Albert O. Hirschman of the Banco de la Republica, Bogota; "A Suggested Model of the Distribution of Soviet Investment" by Gregory Grossman of the University of California; and a review and discussion of the other conference papers by William J. Fellner of Yale University.

A conference on entrepreneurship and economic growth, sponsored jointly with the Harvard University Research Center in Entrepreneurial History, was held at Cambridge in November. For this conference the following papers were prepared: "The Entrepreneur in the Industrial Revolution in Britain" by Charles Wilson of Cambridge University; "Entrepreneurship in the Initial Stages of Industrialization, with Special Reference to Germany" by Fritz Redlich of Harvard University; "Entrepreneurship in Periods of Rapid Growth: The United States in the 19th Century" by John E. Sawyer of Yale University; "Entrepreneurship in Periods of Rapid Growth: Japan" by John Pelzel of Harvard University; "Entrepreneurship in Advanced Industrial Societies: The United States" by Arthur H. Cole of Harvard University; "Entrepreneurship in Advanced Industrial Societies: France" by David S. Landes of Columbia University; and "Achievement Norms and the Motivations of Entrepreneurs" by Francis X. Sutton, now of the Ford Foundation.

Plans concerning possible publication of the papers prepared for the October and November conferences are under discussion. The committee is to meet in December to complete plans for its program during the coming year.

MATHEMATICAL TRAINING

OF SOCIAL SCIENTISTS

William G. Madow (chairman), E. P. Hutchinson, Jacob Marschak, George A. Miller, Frederick Mosteller, Robert M. Thrall; *staff*, Elbridge Sibley.

At a meeting on November 6-7 the committee completed plans for the two eight-week institutes in mathematics for social scientists to be held next summer. The institute at Stanford University will be directed by William G. Madow, and that at the University of Michigan by Robert M. Thrall. Applications for admission, and for a limited number of study grants, must be filed with the Council's Washington office by January 17, 1955, for consideration by the committee in the course of the following month.

As an integral part of its efforts to establish more effective communication between the social sciences and mathematics, the committee has under way several projects for the preparation of teaching and reference materials relevant to particular social sciences. Samuel Goldberg of Oberlin College has completed a draft of a monograph on social science applications of difference equations. Robert R. Bush of Harvard University, Robert P. Abelson of Yale University, and Ray Hyman of Harvard University have prepared a 390-page manuscript giving mathematical examples and problems for psychologists, developed as an adjunct to mathematics texts which do not now include comparable exercises, and including a bibliography. Each problem presented is based on material in the existing psychological literature. Harold W. Kuhn of Bryn Mawr College is preparing a manuscript, including exercises, on applications of the theory of games and linear programming in economics. Gerard Debreu of the Cowles Commission for Research in Economics is developing materials for the study of certain basic economic problems from the algebraic and topological point of view. Funds were provided the Mathematical Association of America to assist in defraying the expenses of a group working at the University of Kansas in the summer of 1954, under the direction of W. L. Duren of Tulane University and G. Baley Price of the University of Kansas, on the preparation of experimental text materials for a general freshman course in mathematics that would be more suitable for students planning to enter fields other than engineering.

The manuscripts by Goldberg and by Bush, Abelson, and Hyman will be used as part of the teaching materials in the 1955 summer institutes, but neither these nor the materials prepared by Kuhn and Debreu are now available for general distribution. Mr. Goldberg is revising and expanding his manuscript for later publication, and it is probable that Mr. Kuhn's manuscript will also be published as a book. Part I of a volume entitled *Universal Mathematics*, based on the work of the Mathematical Association group last summer, has been published in a preliminary edition by the University of Kansas; Part II is not yet available for general distribution.

PUBLICATIONS

COUNCIL BULLETINS AND MONOGRAPHS

- The Social Sciences in Historical Study: A Report of the Committee on Historiography*, Bulletin 64, July 1954. 191 pp. Paper, \$1.75; cloth, \$2.25.
- Research on Labor Mobility: An Appraisal of Research Findings in the United States*, Bulletin 65, by Herbert S. Parnes. October 1954. 216 pp. \$1.75.
- Labor Mobility in Six Cities: A Report on the Survey of Patterns and Factors in Labor Mobility, 1940-1950*, prepared by Gladys L. Palmer, with the assistance of Carol P. Brainerd, for the Committee on Labor Market Research. June 1954. 191 pp. Paper, \$2.25; cloth, \$2.75.
- Adjustment to Physical Handicap and Illness: A Survey of the Social Psychology of Physique and Disability*, Bulletin 55, revised edition, by Roger G. Barker, in collaboration with Beatrice A. Wright, Lee Meyerson, Mollie R. Gonick. April 1953. 456 pp. \$2.00.
- Social Behavior and Personality: Contributions of W. I. Thomas to Theory and Social Research*, edited by Edmund H. Volkart. June 1951. 348 pp. Cloth, \$3.00.
- Support for Independent Scholarship and Research by Elbridge Sibley*. Report of an inquiry jointly sponsored by the American Philosophical Society and the Social Science Research Council. May 1951. 131 pp. \$1.25.
- Area Research: Theory and Practice*, Bulletin 63, by Julian H. Steward. August 1950. 183 pp. \$1.50.
- Culture Conflict and Crime*, Bulletin 41, by Thorsten Sellin. 1938; reprinted September 1950. 116 pp. \$1.00.
- Tensions Affecting International Understanding: A Survey of Research*, Bulletin 62, by Otto Klineberg. May 1950. 238 pp. Paper, \$1.75; cloth, \$2.25.
- Labor-Management Relations: A Research Planning Memorandum*, Bulletin 61, by John G. Turnbull. October 1949. 121 pp. \$1.25.
- The Pre-election Polls of 1948: Report of the Committee on Analysis of Pre-election Polls and Forecasts*, Bulletin 60, by Frederick Mosteller, Herbert Hyman, Philip J. McCarthy, Eli S. Marks, David B. Truman,

with the collaboration of L. W. Doob, Duncan MacRae, Jr., F. F. Stephan, S. A. Stouffer, S. S. Wilks. September 1949. 416 pp. Paper, \$1.75; cloth, \$2.25.

OTHER BOOKS

- America's Resources of Specialized Talent: The Report of the Commission on Human Resources and Advanced Training*, prepared by Dael Wolfe, Director. New York: Harper & Brothers, September 1954. 350 pp. Cloth, \$4.00.
- Labor Mobility and Economic Opportunity*. Related Essays by E. Wight Bakke, Philip M. Hauser, Gladys L. Palmer, Charles A. Myers, Dale Yoder, and Clark Kerr, with a Preface by Paul Webbink. Sponsored by the Committee on Labor Market Research. Cambridge: The Technology Press, and New York: John Wiley & Sons, July 1954. 125 pp. Cloth, \$3.50.
- Psycholinguistics: A Survey of Theory and Research Problems*, edited by Charles E. Osgood and Thomas A. Sebeok. Report of the 1953 Interuniversity Summer Research Seminar, sponsored by the Committee on Linguistics and Psychology. Indiana University Publications in Anthropology and Linguistics, Memoir 10. Bloomington, Ind. October 1954. 214 pp. \$2.50.
- Civil-Military Relations: An Annotated Bibliography, 1940-1952*. Prepared under the direction of the Committee on Civil-Military Relations Research. New York: Columbia University Press, May 1954. 154 pp. \$2.00.

COUNCIL PAMPHLETS

- Bibliographies on Personality and Social Development of the Child*, Pamphlet 10, compiled by Christoph Heinicke and Beatrice B. Whiting. June 1953. 138 pp. \$1.00.
- Exchange of Persons: The Evolution of Cross-Cultural Education*, Pamphlet 9, by Guy S. Métraux. June 1952. 58 pp. 50 cents.
- The Council's bulletins, monographs, and pamphlets are distributed from the New York office of the Council.

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230 PARK AVENUE, NEW YORK 17, N. Y.

Incorporated in the State of Illinois, December 27, 1924, for the purpose of advancing research in the social sciences

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SUMMER RESEARCH TRAINING INSTITUTES

IN the June *Items* the Council announced that it would consider proposals for summer research training institutes in which small groups of postdoctoral social scientists might become acquainted with certain research techniques, methods, or areas of theoretical and substantive knowledge not previously assimilated. It is assumed—and confirmed by the experience of the summer institute in mathematics sponsored by the Council in 1953—that many social scientists who are generally well prepared and firmly committed to research recognize the deficiency of their training in particular respects, and could derive considerable benefit from a summer devoted to filling such gaps.

A typical institute is conceived as bringing together for about two months during the summer a group of perhaps 10 or 12 scholars who, although not necessarily all from the same discipline, have a common background of training and experience sufficient to permit easy communication among them, and who share an interest in research on related problems or in the use of similar methods and techniques. The institute would be directed by one or more research workers who are recognized leaders in particular fields. The Council would defray the direct costs of operating the institute and grant stipends to participants who might otherwise not be able to attend because of their dependence on earnings from teaching or other summer activity.

The need in some cases may be for instruction in techniques that cannot be mastered readily by independent study, or that have been so recently developed that they have not yet been codified and incorporated in the literature. In other cases the chief desideratum may be guided experience in the practical application of research methods or theory. Thus different proportions of formal instruction and "workshop" activity might be appropriate in different institutes. Where the latter element predominates, it will be essential either that the participants bring with them research materials on which they are working, or that the host institution provide materials for analysis or facilities for observation or experimentation.

At a recent conference held by the Council on the present status of research training in the social sciences and ways of improving it, the summer institute program was considered at length. The participants, who included persons actively concerned with research training activities in a number of universities, indicated their belief that summer institutes should be considered as supplementing regular academic programs rather than as substitutes for them. Numerous potentially

fruitful subjects for summer training institutes were suggested, some of which are mentioned below. It is quite evident, however, that the indispensable first steps toward organizing a useful institute are: (1) to identify sufficiently homogeneous groups of scholars interested in participating, and (2) to find institutions and persons willing to offer the desired programs. Thus the list of topics in the following paragraph is to be regarded as only illustrative, and not exclusive.

Possible subjects of formal instruction in research training institutes range from such specific techniques as scaling, factor analysis, or other statistical methods, to fields such as demography in which both substantive materials and special methods are to be learned. In some other fields, critical review of selected research projects conducted by workers in other disciplines might best serve as an introduction to new areas of theory and method. For example, sociologists or social psychologists might gain new insights from a review of certain studies that have been made by psychiatrists or other clinically oriented investigators. Or individuals whose experience has been confined to field observation might similarly be introduced to the theoretical and methodological aspects of laboratory experimentation, or vice versa.

Opportunities for well-guided experience in the application of research methods and theories which have already been learned in the abstract might be made available along with some formal instruction at university or other research centers that conduct sample surveys. Research workers might also be given opportunity to observe the process of research design in an established research organization; or an institute might take the form of a clinic in which participants could consult with more experienced research workers on current problems in designing research.

It is hoped, as stated in the original announcement, that individuals and groups of scholars will take the initiative in making known their interests, so that the Council may endeavor to organize institutes for which there is real need. Negotiations with prospective host institutions concerning the organization of institutes will be feasible only when sufficient demand for a particular program is assured.

Whether one or more institutes will be held in the coming summer will depend upon the nature and volume of early response to this announcement. Inquiries or proposals should be addressed to the Washington office of the Social Science Research Council, 726 Jackson Place, N. W., Washington 6, D. C.

